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THE "ROMAN BOWL FROM BAGDAD"

IN the last volume of this JOURNAL, pp. 310 ff., Professor O. S. Tonks published, under the title "A Roman Bowl from Bagdad," a marble bowl with sculptured reliefs and inscriptions rudely incised. A glance at these inscriptions was sufficient to show that they were based, as Professor Tonks had seen in part, on an Arabic original. As my historical studies have made me for some time interested in the question of the transliteration from Oriental to Western languages and *vice versa*, I took up their study with eagerness. An almost complete decipherment was the result, but with this went the discovery that the inscriptions were forgeries. The brief note pointing this out, which was sent to this JOURNAL, arrived too late for the last number. Professor Fowler suggested that I utilize this delay for correspondence with Professor Tonks, and I have also availed myself of the discussion by friends more competent than myself. By thus going over the main points of controversy in private, we hope to obviate the necessity of reply and counter reply in print.

Our main concern in this paper will be these inscriptions. They were in large part deciphered by Professor Tonks, and his views in regard to them at that time will be seen from the following extracts: "The first word, Sbigmos, is more or less unintelligible. It has, however, a magical sound, and this, with other features in the inscription, led Professor Prentice and myself to look for concealed meanings in the inscription. Indeed, one is ready to look for magical formulae in almost any blind combination of letters after reading such as the following. . . . Then comes a combination of letters which if anything seemed magical to Professor Prentice and myself. The alpha and omega instantly recalled the regulation abbreviation for the Divinity, while the kappa sigma easily expanded

into Kurios. The next letter, which looks like a cross-barred theta, of course has no right to be here if it is that letter. But this form occurs in Christian inscriptions apparently as a punctuation mark, as well as the symbol for God. Alpha might stand for Adam, who appears in the magical papyri in the cosmogenic sense to concentrate the natural forces invoked, or for Abraham, whose name also is used in invocations. So out of $\Lambda\Omega\aleph\Theta A$ we could get the formula Alpha omega, Kurios, Theos, Adam or Abraham. To us that might not mean much; but for a late Greek or Roman who believed in the potency of mystic names this formula would be a very powerful one" (pp. 315 f.). "The presence of Asclepius and Hygieia hints at an association with medicine." "There seems to be a suggestion of magic in the formulae written on the bowl, and inasmuch as this is intimately connected with the practice of medicine, we have, so it would appear, some reason for thinking that the bowl was used for medicinal purposes" (p. 320). From this it appears that Professor Tonks was inclined, in spite of doubts, to the view that the bowl and its inscriptions had magical suggestions. In this connection it is proper to add the following from Professor Prentice: "I never believed that 'these inscriptions were of a magical nature.' . . . The search for magical formulae in such combinations of letters was natural enough to me; but here it was unsuccessful, and the idea was abandoned. I made other suggestions to Tonks about the inscriptions, but the 'partial decipherment' . . . was Tonks' own."

Such, then, is the view hitherto held. It will be most convenient if it is here stated that the present writer would read the main inscription as representing approximately the following Arabic, *سابينة سويرس سبتيموس اوگسطه*, which in turn would be transliterated SBYTMWS SWYRS SABYNA AWKSTĀ, and which would mean Septimius Severus, Sabina Augusta.

Now let us take up the decipherment letter by letter. We have given the Greek on the bowl, the Arabic from which it was transliterated, and the European language, Greek, Latin, French, or what not, from which this Arabic was derived and which goes back ultimately to the Latin, not the Greek, forms. There could be no question as to the proper representation of

the *s*-sound in all three. But the following *e* is not found in the Greek. Evidently it was not in the Arabic, and this indicates that there was no vocalization here. The use of a *b* for the *p* of Septimius simply shows that it has gone through the Arabic, which has no proper *p* and so must use a *b* in the transliteration of proper names. The *i* might be taken as a vowel, inserted to make more easy the pronunciation of two consonants coming together, a not uncommon phenomenon in Syrian Arabic, but it would seem to be better to accept the theory of Dr. J. P. Peters, Professor Torrey, and Dr. B. B. Charles, that we have here the ambiguous group *ya ta*, which, if written in manuscript, as in the reproduction of Dr. Charles, might as easily be read YT, as did our forger, as TY, as it should have been. Incidentally, it proves that the Greek was transliterated from a written and not an oral Arabic. The *gamma* is simply an error of the engraver of the bowl, for what is needed here is a *tau*. The question has been raised by Professor Brünnow as to why we have here the use of a *ta* and not a *ṭa* indicated in the Arabic text. This may be phonetic, due to the following *i*, or it may be, as suggested by Dr. Charles, that it is folk-etymology, due to an assumed connection with the Arabic *sebt*, Sabbath, a not unlikely theory when we remember that our forger was in all probability a Christian. But such assumptions are by no means necessary, for there are plenty of examples in modern Arabic of the variant use of *ta* and *ṭa* in the same word, and, indeed, Professor Torrey tells me that this was true in the Arabic spelling of his own name. The *omega* indicates a *waw* in the Arabic. This could hardly stand for an *omicron*, which is not likely to have been represented in an unvocalized text. The more natural supposition is that this *waw* represents a Latin *u*, which would then point to the Latin, not Greek, form of the word. Professor Torrey has drawn attention to another possibility, that the *waw sin* is a natural contraction for *ius*.

Professor Tonks has successfully made out the second word as Severus, and the third as Sabina. Professor Torrey believes that the *alif*, which must be assumed as the original of the second letter in Severus, is what we should naturally expect as a result of the genius of the language. He accordingly thinks

that my first explanation of this as a hint of vocalization, due to the misreading of a *fatha* as short *a* instead of short *e*, is improbable. The argument from the general character of the language is in his favor, and we have no other certain trace of vocalization. But it should be noted that the form he postulates is not found among the variant spellings of the name of Severus, which Dr. Charles has found for me in Mas'udi. As I know from my own experience, it is not difficult to pick up a lithographed copy of Mas'udi in Syria, and the man who was responsible for the Arabic forms could easily have learned the literary forms, for he certainly was well educated. The *omega* represents another *waw*, which in its turn can hardly stand for *omicron* *upsilon*, as our forger seems to have mechanically transliterated each single letter of his Arabic original by a single letter in the Greek. Then the only other possibility seems to be a *u*, and this again points to an original which used the Latin, not Greek, form. On the other hand, the use of *ya* to represent the second *e* in Severus, which is long, would with difficulty be assumed as known to a modern Arabic-speaking person. In the Greek form, the *eta* would, of course, permit this, but there is no other indication that there was a Greek basis for these names. The most probable theory is to take the use of the literary forms of Severus already in Arabic, for some of these actually do have the *ya*.

We have no more points of interest until we note the *alpha omega* for *alif waw* as the natural representative of *Au*. The man who transliterated the *g* of Augusta by *kaf* evidently lived where the *gim* was not pronounced as hard *g*, or he would have used that letter. If, however, he had followed the normal literary fashion, he would have used *gh*, which we find in Aughustus. But such transliteration of the foreign *g* by *kaf* is common in Syria, while the Persian has actually taken this *kaf*, given it a special mark, and made it thus a *g*. The cross-barred *theta* is one of the best proofs of the forged character of the inscriptions, for this archaic form could not possibly have been used within centuries of even the date pointed out by the imperial names. We might argue that this use of the *theta* represents a *ta*, and that this was used because it so frequently occurs in Aughustus. But we have already seen that

our form does not agree with the literary form in regard to the *g*, and so perhaps we had better not attempt to find the reason for this *theta* in literary reminiscences. Here best fits Professor Torrey's seductive theory, that the man who engraved the bowl mechanically picked out his Greek letters by means of one of those tables of alphabets, so common in Syria, where, opposite the Arabic forms, are to be found the main types from cuneiform down.

The remaining inscriptions consist mostly of names, and have been in general sufficiently worked out by Professor Tonks. Certain additional points, however, may be mentioned. The Askolab undoubtedly goes back to the Latin form Asculapius rather than to the Greek Asklepios, especially as the *omega* here must again represent a *u* through a *waw*. But it is quite possible, as Professor Torrey suggests, that it is not really the actual Latin form that we are to assume as the basis, but the modified French form Esculape, the pronunciation of which would, of course, exactly coincide with the Askolab. French is so well known in Syria, and especially in the Lebanon district, that it requires no violent assumption to believe this. And we could then find in the Eigi over the figure of Hygieia another proof of French influence. Certainly the *gim* must here go back to something other than hard *g*, for that we have already seen was represented by *kaf*. But in the Lebanon *gim* would regularly have the *zh* sound, and so we would have the exact reproduction of the pronunciation of the French Hygie. When Professor Torrey suggests that the use of *E* for the Arabic *ha* is due to this being the representative of that letter in the table of alphabets, he may well be right, but we should also consider the possibility that this loss of *h* may be due to the loss of the aspirate in the modern Greek. He reads the character after BNΔΩPA as *l*, and explains it as an ending for that name in *alif ya*. Finally, he has been able to make out the Aoginon, which had eluded Professor Tonks and myself, as Eugenius, a late ruler whose coins are not uncommon in Syria. His explanation of the second N as due to a confusion of the manuscript form of final *sin* with that for *nun* seems to solve that difficulty.

Such, then, is a brief account of the decipherment as it stands

at present. However much we may differ in details, none of the scholars to whom I have shown this has any doubt of the inscriptions being forged, and, indeed, Professor Tonks has admitted this. But he still insists that "the admission of their falsity does not imply necessarily that the bowl is a forgery," and that he "is still forced to believe that, although the bowl presents certainly an unusual appearance, and has quite as certainly been tampered with, its authenticity must be granted, grudgingly perhaps, until some further evidence turns up to render its falsity indisputable." But the mere fact that there is something admittedly false about the bowl leaves it open to suspicion, and the burden of proof is thrown upon the scholar who would still defend its authenticity. The bowl *may* be genuine; one would hesitate to decide without a more careful examination, and that of the original, than is possible for most of us. But it must and will remain suspect until strong proof to the contrary be given. Professor Tonks himself has seen that there are other reasons to suspect the bowl. He is "tempted . . . to say that . . . the whole bowl is modern," "that there would seem much reason for relegating the work to the category of forgeries," and "the first impression of the bowl was such as to cause one to be suspicious of it." To be sure, he feels himself able to answer these objections, and yet our suspicion is, to say the least, aroused. Professor Prentice, though he does not consider that my "theory about the inscriptions affects the question of the age of the bowl itself" because "it has been evident from the beginning to all of us here that the inscriptions are of different origin from the bowl itself, even when some of us believed that both might be genuine, *i.e.* ancient," that "the inscriptions are rudely scratched while the bowl and its figures are carefully carved," though he feels that *if* the wolf children are really different from the Renaissance children, then the bowl is old, nevertheless states that he has "never been convinced of the genuineness of the bowl, and is not now." And Professor Brünnow also declares, "From the very first time I laid eyes on the bowl, I had a very strong suspicion that it was a modern forgery." When those who have actually seen the bowl have their "strong suspicions," we may be also permitted to have our doubts.

By publishing the bowl under the heading of "A Roman Bowl from Bagdad," Professor Tonks has given a certain amount of approval to the statement of the person from whom it was bought that it came from that place, though in the text he qualifies this by "was said to have come from Bagdad," and adds a note in which it is stated that Père Jaussen had been told that it came from the Hauran. The assumption that it came from Bagdad has serious difficulties, as has also the theory that the inscriptions are magical, and that the bowl was, therefore, used for medicine. If we are to accept these theories, we naturally turn, for comparison, if we are acquainted with the archaeological productions of Babylonia, to the famous Mandaean bowls which have also magical inscriptions and designs. But, though they belong to the very period and locality from which this is supposed to have come, they are totally different, and in this I am confirmed by Professor J. A. Montgomery, the one authority on these bowls in America. It is difficult to assume that these reliefs can be attributed to as early a date as the third century, and Professor Tonks feels it necessary to "come down to Coptic times to find parallels." But it is still more difficult to assume the production of such strongly pagan reliefs, even in Seleucia, so long after the accession of the bigoted Sassanidae in 226 A.D. So, for this period, Coptic parallels cannot be applied to Babylonian products. There is, of course, much less difficulty in assuming Coptic parallels to productions of central Syria and the Hauran, though even here we should have to be cautious in applying such parallels. Professor Tonks writes, "I do not believe the provenance given to be important." But it is important to this extent, that what would be a probable parallel in Egypt becomes much less probable in the Damascus region, and most improbable in Babylonia.

In the earlier draft, I had ventured a few remarks on the subject of general style. But now I can rely on the remarks of no less an authority than Studniczka, in the *Kunstchronik*, XIII (1911), 112: "Den Herausgeber selbst, obgleich er zugleich der Eigentümer zu sein scheint, hat der Verdacht moderner Entstehung sehr beschäftigt. Wie berechtigt er ist, lässt sich ohne Prüfung des Originals nachweisen. Ein

Hauptstück des Relieffrieses ist die auch von Tonks erkannte Nachbildung der altetruskischen Bronzewölfin im Konservatorenpalast, die sonst auf keinem antiken Bildwerke vorkommt. . . . Und zwar erscheint sie auf der Marmurvase mitsamt den in der Renaissance hinzugefügten Zwillingen. Die von Tonks . . . hervorgehobenen, sehr leichten Abweichungen der Kinderfiguren im Relief von denen der Bronze können daran nichts ändern. Die nach Petersens einleuchtender Beweisführung ursprünglich zu der kapitolinischen Wölfin gehörigen, verlorenen Zwillinge müssen ganz anderen, archaischen Stiles gewesen sein. Nach dieser einen Feststellung ist es kaum nötig, noch all die anderen Gründe für die Unechtheit des Marmorwerkes anzuführen, die ja zum Teil der Herausgeber selbst schon beigebracht hat."

Professor Tonks, however, still holds to the authenticity of the engraved figures, and I accordingly present his view of the present situation: "One is bound to respect the dictum of so distinguished a scholar as Studniczka. But in spite of this, I cannot help feeling that since the maker of this bowl has been so literal in the rendition of the wolf he would not have been likely suddenly to change and take liberties with the twins. That being the case, the difference from the Renaissance pair that the twins present in the matter of the position of the arms must have value. Its close resemblance to the original postulates that the copy must have been made by one who could study the wolf closely while carving the bowl. This would mean that the maker was at the time in Rome (or, what is unlikely, possessed an accurate copy of the Roman wolf). The fact that the twins differ from the modern children shows that the bowl was created before the original group was lost,—unless, and this is unlikely, the maker had a very perfect copy of the wolf with the original children in place. The bowl, therefore, must date between the date of the disappearance of the original twins and that of the busts. In view of this, I am bound to say that unless other proof of falsity is given, we must accept this group as the most important document we have for the character of the original twins. In regard to the other matters touching the authenticity of the bowl,—as to these I have given my reasons for believing them valuable, so

that it remains for those who believe the bowl a forgery to refute them. In other words, I believe that the burden of proof now rests upon these scholars and no longer upon me. The question of the inscriptions and of the bowl itself must be considered apart. That modern forgers are apt to 'improve' genuine antiques is of course well known."

I have thus, in attempting to sum up the matter, presented in their own words the contentions of both sides. But one point still remains for discussion. Studniczka adds "*Zu der dennoch wünschenswerten Quellenanalyse der Fälschung fehlt mir die Zeit.*" In my own case, it is rather a lack of knowledge and especially of proper library facilities. Yet it is important, if this is a forgery, and as in a way a proof of forgery, if we can to some degree indicate the possible sources of the forger.

As for the bowl itself, it at once strikes the man who has travelled in the Orient as being most probably an example of that commonest of all types of forgeries, the reproduction in the more expensive stone of something found in a cheaper material. It is unfortunate that we do not have a closer determination by a geologist of the stone, for this would in itself give a practically certain clue to the locality from which it came. As to the figures, the heads of the imperial couple seem to me, as well as to several others, to have been taken from coins, though it is quite possible that the direct source was some book or other printed matter. Professor Torrey also notes, what must be familiar to any one who has collected coins in Syria, that coins of Severus and Sabina are common enough there. But, though similarities may be traced between our heads and the coin figures of these rulers, there is no more reason to assume that the forger really knew that these represented Severus and Sabina than that he guessed right in the case of the "Atalanta." To me, as well as to the majority of those with whom I have discussed the question, all the figures appear to have been taken from some book. Needless to say, it would be foolish to insist on any one book or group of books, so long as we are unable to place our hands on any actual original. Nor should we forget that the man who could make, or find, such accurate transliterations into Arabic as we have

noted, need not, and probably would not, have confined himself to one book. In his library, for use in giving both the names and the figures, he may have had some of the books on ancient history which are listed in the catalogues of the American and Jesuit Presses at Beirut; he may have used the Arabic Oriental periodical, *al Machriq*; he may have had a somewhat elaborately illustrated history of Syria, which I once saw and have since been unable to get track of; or he may have used, as certain indications make possible, French works, in which case a wide field is opened to our conjecture. Without a larger collection of printed material than is here accessible to me, I cannot pretend to settle the matter. I feel certain that the originals of the figures will sometime be found in some book or books, but what these are, or are not, I shall not attempt to settle.

Somewhat more exact results may be obtained as regards the inscriptions. Whether one or two or a dozen men were engaged, whether there was a "learned friend" who gave the forger the Arabic forms, or whether the man who wrote the Arabic was the same as the man who wrote the Greek, and whether this man or these men was or were the same as the man who carved the bowl itself, cannot be proved, and these points are comparatively unimportant. We can be sure that the bowl was carved by a man who knew little Greek, who had learned this perhaps only by the use of a table of alphabets, perhaps only through the knowledge gained by the church services, if he was, as is at least possible, a "Greek" Christian. On the other hand, the Arabic from which this Greek has been transliterated is perfect. This matter is so important that I cannot do better than quote Professor Torrey, as a far greater authority than I, on this point. "It is not a painful transferring, letter by letter, such as the Arabic-into-Greek performance is; nor can it possibly be explained by supposing any such process. On the contrary, it is at every single point just the way in which a man who could read the European languages easily would ordinarily render these names. He uses everywhere the conventional equivalents, not at all the same as those which would have been used necessarily by an ignorant man. This statement is fundamentally important, and I am ready to support it in detail, if there is any need of doing

so." We must assume, then, as proved that the Arabic from which the Greek was transliterated was written, and that it ultimately goes back to a printed source. This may have been taken by the forger from the written notes of a "learned friend." It may equally well have come from the books in Arabic or in French in the library of a man who read these two languages with facility, but whose knowledge of the Greek might be confined to what he could painfully pick out from the table of alphabets which we should quite expect to be found in the library of such a Syrian amateur.

No attempt is made to underestimate the difficulties to be met by the belief that the bowl is forged, and, unless we find the actual originals, those who still insist upon the authenticity of the bowl itself may find grounds for their opinion. These difficulties increase when an attempt is made to explain how the forgery came into being, for here we have a wide range of possibilities, though limited to a certain extent. But, when all deductions are made and when the difficulties for and against are stated, the greater difficulties are found in the assumption that the bowl is genuine. It is admitted by all that the inscriptions are forgeries, and this, in spite of the statement of Professor Tonks, does throw a suspicion on the bowl, even if it were otherwise without such suspicion. When to this natural suspicion we add all the other objections, it becomes difficult to believe in its genuineness. Professors Brünnow and Prentice have seen the bowl and are still not persuaded that it is genuine. Professors Bauer, Fowler, Jastrow, Montgomery, and Torrey have all, for one reason or another, declared their belief that it is a forgery, and we have the authority of Studniczka to the same effect. But, as there are still those who believe that it is at least probable that the bowl is genuine, this somewhat wearisome discussion of its authenticity may have had its use.

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